Year: 2016

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DOI: https://doi.org/10.1098/rspb.2015.1439

Originally published at:
Oud, Bastiaan; Krajbich, Ian Michael; Miller, Kevin; Cheong, Jin Hyun; Botvinick, Matthew; Fehr, Ernst (2016). Irrational time allocation in decision-making. Proceedings of the Royal Society of London, Series B: Biological Sciences, 283(1822):20151439.
DOI: https://doi.org/10.1098/rspb.2015.1439

Irrational Time Allocation in Decision Making

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B.O., I.K. and E.F. designed study 1. B.O. and I.K. conducted study 1. B.O. analyzed data of studies 1, 2 and 3. K.M., J.C., M.B. and I.K. designed study 2. K.M. and M.B. designed study 3. K.M. and J.C conducted studies 2 and 3. B.O., I.K., E.F., K.M., J.C. and M.B. wrote the article.

Key words: decision-making, speed-accuracy tradeoff, neuroeconomics, sequential-sampling model, evidence accumulation, optimality, time allocation

Acknowledgements:
The research undertaken at the University of Zurich is part of the advanced ERC grant "Foundations of Economic Preferences" and the authors gratefully acknowledge financial support from the European Research Council. The research undertaken at Princeton University was funded by the National Science Foundation (CRCNS 1207833) and the John Templeton Foundation. The opinions expressed in this publication are those of the authors and do not necessarily reflect the views of the funding agencies.

Competing interests statement:
We have no competing interests.

Abstract
Time is an extremely valuable resource but little is known about the efficiency of time allocation in decision making. Empirical evidence suggests that in many ecologically relevant situations, decision difficulty and the relative reward from making a correct choice, compared to an incorrect one, are inversely linked, implying that it is optimal to use relatively less time for difficult choice problems. This applies, in particular, to value-based choices, in which the relative reward from choosing the higher-valued item shrinks as the values of the other options get closer to the best option and are thus more difficult to discriminate. Here, we experimentally show that people behave sub-optimally in such contexts. They do not respond to incentives that favor the allocation of time to choice problems in which the relative reward for choosing the best option is high; instead they spend too much time on problems in which the reward difference between the options is low. We demonstrate this by showing that it is possible to improve subjects' time allocation with a simple intervention that cuts them off when their decisions take too long. Thus we provide a novel form of evidence that organisms systematically spend their valuable time in an inefficient way, and simultaneously offer a potential solution to the problem.

We all know the phrase "time is money", and yet at some point or another many of us have caught ourselves agonizing too long even where it makes little difference what we choose, such as what to order for dinner at a restaurant or what movie to watch. Far from being a uniquely human problem, many species exhibit such behavior. Naturally the question arises whether this phenomenon is simply an unlucky outcome of an optimal decision-making process, or whether the process itself is sub-optimal. Much work in decision science has focused on whether organisms achieve optimal decision outcomes (e.g. [1–4] and much of the experimental economics literature) but relatively little attention has been paid to how they allocate their time while making decisions.

The problem arises due to the well-known speed-accuracy tradeoff, where more time invested into a decision yields a more accurate response [5,6]. One explanation for this phenomenon in many choice contexts is due to the way the brain gradually accumulates noisy evidence for the different choice options, up to predetermined thresholds. Theoretical work has shown how speed-accuracy tradeoffs can optimally be resolved [7–9]. For example, when choice difficulty and the benefit of a correct response are held constant, the drift-diffusion model (DDM) is known to be optimal [10,11]. By optimal we mean that for a desired accuracy rate the DDM minimizes the expected response time (RT) [12]. Recent years have seen much research showing that organisms including flies[13], ants[14], bees[15–18], rats[19,20], primates[21–29] and humans[30–35] use sequential sampling model (SSM) processes (like the DDM) to make many decisions, and that they do respond to speed or accuracy constraints. Moreover, these models apply not only to many perceptual decisions, where there is an objectively correct response, but also to several value-based decisions, where the correct answer is based on subjective preference [36–50].

For instance, the house-hunting behavior of ants and bees is an example of collective value-based decision-making where individual organisms evaluate the suitability of potential nest sites and then recruit and compete with other members of the group in order to guide the final choice of the whole colony. Ants recruit by physically leading, and eventually carrying each other to attractive nest sites, while bees use a "waggle dance" and head butting to communicate the location of attractive hive sites and inhibit bees favoring other sites, respectively. In both cases, these scouts recruit other colony members more readily for higher quality sites, and so support builds more quickly for better options. Once there is enough support for a particular location, the decision is made and the entire colony picks up and moves. This collective behavior is governed by the speed-accuracy tradeoff; colonies may emphasize either speed or accuracy depending on the urgency of the situation.

More typically, SSMs are applied to individual decision-making behavior. For instance, an animal may have to quickly evaluate the attractiveness of various potential foraging sites based on the likely quantity and quality of food available, exposure to predators, distance away, etc. [51,52]. An overemphasis on accuracy may demand an unreasonable amount of time to evaluate the potential foraging sites, while an overemphasis on speed may lead the animal to a poor site.

This literature on the optimality of time allocation has mainly focused on the simple case where difficulty and the relative reward for a correct response (compared to an incorrect response) are both held constant, but in the real world these can vary [53,54]. One point that has not been widely acknowledged is that in many ecologically important situations, difficulty and relative reward are in fact linked. In particular, in value-based (economic) decisions where the individual receives the item that he chooses, the benefit of making the correct decision decreases as the options get closer together in subjective value. Simultaneously, as this occurs the items become harder to distinguish, and we know from the SSM literature that mean decision time increases. As a result, more difficult choices generally take longer, even though the correct choice yields only a minor increase in benefit over the incorrect choice. For example, a foraging animal might find itself torn between two equally attractive patches, wasting time that could better be spent quickly sorting the edible items from the rest.

In settings like these, the optimization problem becomes more complex and there is no clear way to determine whether decision makers are behaving optimally. In the case of fixed difficulty it is optimal for the decision maker to accumulate evidence until the total net evidence reaches a constant threshold [55]. However, when the relative reward for making the correct decision is tied to the difficulty level, the decision-maker can update his/her prior about the subjective-value difference between the options in this particular trial based on how long the decision has taken so far. The decision-maker should realize that as time goes on, the expected relative benefit of making the correct decision is decreasing. When there is limited time to make many decisions, time spent on a low-benefit decision represents an 'opportunity cost'[56]. Thus, when the benefit of making the correct choice differs across choice problems, the decision maker should re-allocate time away from the problems where the relative rewards are small and more time toward the problems where the relative rewards are large. Prior work, for instance in similar settings where information acquisition is increasingly costly over time [57,58], indicates that these situations call for the decision thresholds to decrease over time within a trial [55,59–63]. In the neuroscience literature, collapsing-threshold models (and similar urgency models [60,64]) have been gaining popularity, though the behavioral evidence for them is mixed [65,66].

It is critical to note that all these models nevertheless predict that decisions between similar options ("hard" choices) will on average take more time than between dissimilar options ("easy" choices). Conceivably, it may be that hard choices are unavoidably slower than easy choices because easy choices can be made more quickly than they can be distinguished from hard choices. That is, it may not be possible to quickly identify the difficulty of a choice problem. Therefore, the observation of RT differences between easy versus hard decisions is not by itself sufficient to establish the sub-optimality of time allocation. Here, we tackle this issue by developing a novel empirical method for testing the optimality of behavior.

We begin with an economic task and then also investigate a perceptual decision-making task that incorporates the difficulty-relative-reward connection that one finds in economic choice. In each task, subjects made a series of choices where time was both scarce and valuable. The first uses naturalistic stimuli and relies on subjects' own valuations, whereas the second uses an approach that affords external control of relative value. While the two studies appear quite different on the surface, they share a very important feature. At the beginning of both studies, subjects had a "baseline" expected outcome that they would earn if they did nothing. By making good choices, subjects could increase their expected earnings from this baseline level. The amount of this increase varied from trial to trial, along with the difficulty of the decision.

The results clearly demonstrate that subjects misallocated their time. We established this by introducing a simple intervention that improved subjects' performance on both decision tasks, using only information that they themselves had available. Importantly, subjects seemed to learn from our intervention and so some of the benefits remained even in the subsequent absence of the intervention. Thus we not only show that decision-makers sometimes wasted their valuable time, but that it was possible to use simple training to help them improve.

METHODS

Ethics Statement

Study 1 was approved by the University of Zurich ethics committee and all subjects gave informed written consent before participating. Study 2 was approved by the Princeton University IRB and all subjects gave informed written consent before participating. Both experiments were conducted using Matlab Psychophysics Toolbox[67].

Study 1

49 subjects provided informed consent and were paid a flat fee of CHF 30 for their participation, plus possible additional cash of up to CHF 2.50 from the first part of the study. Subjects first indicated their willingness to pay (WTP) for 100 different snack foods, using a Becker-deGroot-Marshak (BDM) mechanism [68], which has the property that it is in subjects' best interest to reveal their true WTP (see details below). For each trial, subjects saw a color photograph of the item and a slider bar (with a random starting location) that they could use to select a WTP from CHF 0 to 2.50, in steps of CHF 0.25. Subjects used the "left" and "right" arrow keys to move the slider and the "up" arrow key to confirm their choice.

Subjects then proceeded through five blocks of binary decisions between pairs of these items. Using these WTPs, we constructed choice pairs with known valuation differences between the two items. Each block contained 100 trials, half of which were constructed as "easy/high-stakes" choices (large valuation difference), and half "hard/low-stakes" (small valuation difference). It was impossible to reach every trial in any given block, since each block's duration was 150s, with 1.5s inter-trial interval (ITI). Subjects indicated their decision by pressing the "left" or "right" arrow keys on the keyboard. Critically, subjects were informed that the computer would randomly make any uncompleted choices at the end of the 150s.

At the end of the experiment, subjects were rewarded for one random trial. This trial could be a BDM trial (p=1/6) or a binary choice trial (p=5/6). For a choice trial, subjects simply received the item that they, or the computer, chose on that trial. For a BDM trial, the computer generated a random price between CHF 0 and 2.50. If the random price was equal to or less than the subject's WTP for that item, then the subject received the food and paid the random price (out of an endowment of CHF 2.50). If the random price was above the subject's WTP for that item, then the subject did not receive the food and kept the endowment of CHF 2.50. In addition to these earnings, all subjects earned CHF 30 for their participation in the study.

The first of the five blocks (*T*) was used to obtain an individual empirical distribution function for the response times in the task. Four more blocks followed. Two of these were nonintervention blocks (*N*), which were constructed identically to the first block, but with different choice pairs. The other two blocks were intervention blocks (*I*), in which subjects were reminded on screen to "choose now'' after a pre-specified amount of time had passed. If they did not make a choice within 0.5s of the message, the choice was randomly made for them, and the next trial commenced (after the ITI). The mean deadline was defined for each subject separately, such that it would have cut off the slowest 30% of their decisions in the *T* block. For each subject, there was a 50% chance that they would experience the sequence *T-I-N-I-N*, and a 50% chance that they would experience the sequence *T-N-I-N-I*.

To assess performance on the task, we created a measure of surplus that captures the subjective value generated through making choices, and is analogous to the points earned in Study 2. To do this, we used each individual subject's WTPs to create the following measure:

$$choice\ surplus = \begin{cases} (v_{chosen} - v_L) - \frac{1}{2}(v_H - v_L) & for\ human\ choices \\ 0 & for\ computer\ choices \end{cases}$$

Here, v_{chosen} is the WTP for the chosen item, v_L is the lower of the two WTPs, and v_H is the higher of the two WTPs. Thus, choice surplus represents the degree to which the surplus from actual human choices outperforms chance. Computer choices were treated as performing at chance level (zero by construction), regardless of their actual random realization, to reduce artificial noise in the measure.

Study 2

42 subjects were recruited through a Princeton University online subject recruitment system and provided informed consent to participate in this study. Two subjects were excluded from analysis due to outlier behavior. These two subjects scored more than three standard deviations away from the mean for one of the two conditions, leaving us with an analyzed sample size of 40 subjects. The minimum payment in this study was set to \$12, and the average payment was \$18.29.

The instructions were provided on screen. Subjects were informed that they would be paid \$1 for every 1000 points they earned during the study, rounded down to the nearest dollar. For example, a subject with a score of 17232 points would receive \$17.00. The minimum payment was set at \$12 and the average payment was \$18.29 (all results still hold if we exclude the subset of subjects who scored under 12000 points).

The task was to indicate, using the keyboard, which side of the computer screen contained more flickering dots. The difference in the number of stars between the two sides of the screen was either 10 (hard trials) or 80 (easy trials), with the mean number of stars equal to 100 (e.g., a "hard'' trial had 95 vs. 105 stars, and an "easy'' trial had 60 vs. 140 stars).

The study was divided into five blocks. The first of the five blocks was a 5-minute unpaid trial block (*T*) to familiarize subjects with the task, with an ITI of 0.5 seconds separating trials. The four remaining blocks each took 10 minutes, with subjects earning points that were later converted to cash. On each trial in these blocks, participants could either gain or lose a specific number of points, which we refer to as the "stake'' for that trial. Subjects were self paced and continued to make decisions until the block time was up. The ITI in the paid blocks was 2s, plus the time needed to prepare the next trial, resulting in an average empirical ITI of ~2.2s.

Subjects were informed that the stakes corresponded to half the difference in the number of stars between the two sides of the screen. For example, if there were 105 stars on the left and 95 stars on the right, the stakes were 5 (since (105-95)/2=5). Since subjects did not know the number of dots in advance, on every trial they had to infer the stakes based on the on-screen stimuli.

As in Study 1, there were two within-subject experimental conditions: intervention blocks and non-intervention blocks. Participants were introduced to the two experimental conditions in the following way:

"On some runs [blocks], there will be a deadline. If you do not respond by the deadline, the trial will be aborted, and you will earn no points. A short time before the deadline, the stars will disappear - respond quickly when this happens!''

The mean deadline was determined in the same way as in Study 1. Each trial, the actual deadline was drawn uniformly from within 50ms of the mean deadline.

Analysis

The mixed-effects regressions reported for Studies 1 and 2 use the following model**:** $y = \beta x_{ij} + \nu_i + \varepsilon_{ij}$, where β is a vector of coefficients, x_{ij} is the vector of regressors in trial j of individual i, ν_i is an individual-specific noise term, and ε_{ij} is a general noise term. For Study 2 (Table S2 in the supplement, columns 4-6), the dependent variable y is cumulative surplus per block, in points, since every trial was paid in full (1000 points = 1 USD). For Study 1 (Table S2, columns 1-3), the dependent variable y is the blockwise mean surplus, in CHF per trial. Since there were 100 trials per block, conversion to the block level requires multiplying by 100. Before regressing, the data was first collapsed to obtain blockwise mean surplus for each participant, resulting in four data points per participant (representing blocks 2-5). The mixed-effects regression models for both studies were estimated using maximum likelihood. Standard errors were clustered at the individual level. The first (trial) block was excluded from all analyses.

RESULTS

Here, we report the results of two separate decision-making studies, one using an economic value-based food choice task, and one using a perceptual choice task. Further (consistent) results from a related third study are reported in the supplementary materials. In each study, subjects faced a fixed amount of time to make as many decisions as possible.

In the value-based food choice task (Study 1), subjects had to decide which of two food items they would prefer to eat at the end of the experiment (Fig. 1A). Using the elicited values from a separate valuation task (see Methods), we constructed, for each participant, trials in which the valuation difference was large (easy trials), and trials in which the valuation difference was small (hard trials). In each block there were more decisions (100) than could be made in the time available. Any remaining decisions at the end of the time-limit were made randomly by the computer. At the end of the experiment subjects received the chosen food item from one randomly chosen trial. Crucially, the trial randomly chosen for payment could be a self-made or computer-made decision. Thus the subjects had an incentive to make as many of their own choices as possible, to reduce the chance of receiving a randomly chosen food item.

In the twinkling-stars task (Study 2), subjects had to decide which side of the computer screen contained more dots. The dots disappeared and reappeared at random, giving the appearance of twinkling stars (Fig. 1B), but the underlying number of stars on each side of the screen remained constant throughout a trial. Each trial we varied the difficulty of the task by changing the difference in the number of stars between the two sides of the screen. Analogous to Study 1, a negative link between choice difficulty and relative reward was also induced in Study 2. Here, participants received points according to the difference in the number of stars between the two sides of the screen.

Crucially, in both Studies 1 and 2, there is more to be gained from making the correct choice in easy trials than in hard trials. By construction, trials with a high relative reward are both easier to answer accurately and have a larger impact on the expected earnings, thus constituting a better investment of time than the trials with a low relative reward.

Despite this reasoning, we found that subjects spent significantly more time on trials with a low relative reward than they did on high relative reward trials in both the food-choice task (Fig. 2A) and the twinkling-stars task (Fig. 2B). In the twinkling-stars task, the average, across individuals, of median RT in hard trials was 1.02s, compared to 0.58s in the easy trials. A nonparametric Wilcoxon matched-pairs signed-ranks test rejects the hypothesis that these two values are equal at $p<0.0001$. In the food-choice task, there is a clear association between the absolute difference in willingness to pay ($|\Delta WTP|$) and RT. We see that a CHF 1 increase in $|\Delta WTP|$ corresponded, on average, to a decrease in log(RT) by 0.218 units ($p<0.001$, mixed-effects regression, see Table S1 in the supplement).

The RT pattern displayed in Fig. 2A-B is suggestive, but to truly identify whether behavior is sub-optimal, more direct evidence is needed. To convincingly demonstrate the sub-optimality of this pattern, it is sufficient to demonstrate that unrestricted performance can be improved upon without using any additional information. Specifically, we hypothesized that it might be possible to improve subjects' performance in these tasks by imposing a per-trial deadline on decision-making.

In both studies, subjects were informed that there would be five blocks of decisions and that each block was time-limited. They were also told that in some blocks there would be within-trial deadlines. Being cut off only occurred in 1.60% and 2.14% of intervention block trials, in Studies 1 and 2, respectively. In what follows, we will refer to the blocks with deadlines as intervention blocks (*I*), and those without as non-intervention blocks (*N*).

Comparing the performance on the intervention blocks to the non-intervention blocks (excluding the first blocks), we found that the effect of the intervention was beneficial for 80% of participants in Study 1 and 60% of participants in Study 2. The intervention helped subjects earn significantly more points in the twinkling-stars task ($t(39) = -2.2215$, $p=0.032$, paired) and significantly more value (see Methods) in the food-choice task($t(48) = -4.1973$, $p<0.0001$, paired).

Since we have repeated measures, and each subject went through a total of five blocks, it is possible that gaining experience with the task improved performance. To rule out this possible confound, blocks 2-5 were run either in sequence *N-I-N-I* or in sequence *I-N-I-N*. Fig. 3 shows mean task performance block by block for Studies 1 (panel A) and 2 (panel B). To analyze the benefit of the intervention while statistically controlling for the sequence of blocks, we regressed blockwise performance from blocks 2-5 on both a dummy variable for the intervention blocks, as well as an integer variable (2-5) that encoded the block number. The mixed-effects regression results (see Table S2 in the supplement) show that, all else being equal, with each additional block, earnings increased by 61.86 points ($z=5.90$, $p<0.0001$) in the twinkling stars task. In the food-choice task, value per block increased by CHF 1.33 ($z=6.69$, $p<0.0001$) (if all the trials had been realized; in reality only one trial was realized since we could not give a subject 500 food items to eat). However, the positive effect of the intervention remains significant when we control for experience, with subjects earning an average of 76.28 points more in an intervention block of the twinkling-stars task ($z=-2.11$, $p=0.035$) and a value of CHF 1.69 more in an intervention block of the food-choice task ($z=-3.75$, $p=0.0002$).

Finally, we investigated whether subjects learned from the intervention and so improved in subsequent non-intervention blocks. In order to test for this, while controlling for the effects of experience, we ran a mixed-effects regression (see Table S2, specifications 2 and 5) that included a block-number regressor, as well as dummy variables for intervention blocks and for pre-intervention blocks. The regression results show that performance in post-intervention non-intervention blocks was higher than in pre-intervention blocks, though the effect was only significant in the food-choice task. In the twinkling-stars task, pre-intervention blocks fared 26.32 points worse than the non-intervention blocks that followed ($z=-0.40$, $p=0.68$), while in the food-choice task, pre-intervention blocks fared the equivalent of CHF 3.55 worse (if every trial had been realized) than the post-intervention ones ($z=-4.20$, $p<0.0001$). While intervention blocks continued to outperform post-intervention non-intervention blocks (see Table S2, specifications 3 and 6), this effect was only marginal with a remaining performance increase of 69.58 points per block in the twinkling-stars task ($z=1.64$, $p=0.1$) and CHF 0.75 per block in the food-choice task ($z=1.33$, $p=0.18$).

DISCUSSION

Here we have shown that decision makers are consistently sub-optimal at investing scarce decision time, but that this can be mitigated using a simple intervention where we impose choice deadlines. We observed behavior that failed to maximize earnings when subjects had to decide how to allocate their time across many binary choices. This finding replicated across two separate studies, one involving a food-choice task, the other involving a perceptual decision-making task. These two studies tell a consistent story, in which people apparently misallocate their time, spending too much on those choice problems in which the relative reward is low.

These findings are economically counter-intuitive because we find that imposing an additional constraint (a deadline) onto individual decisions actually improves the overall outcome. Theoretically, the same constraints could have been self-imposed by the subjects.

The fact that our intervention improved performance thus means that behavior was not optimized to maximize earnings in these settings.

This work highlights the fact that the classic speed-accuracy tradeoff is an oversimplification of the typical tradeoffs faced by organisms in their natural environments [16,17,52]. Each choice is not equally important and so rather than trying to maximize accuracy, organisms should be looking to maximize the relative benefits from their decisions. Optimally behaving organisms should know the relationship between strength of preference and RT, and so infer over time that the current decision is less and less worth making correctly. This idea is captured by the well-known paradox of Buridan's ass, where an ass that is equally hungry and thirsty is placed halfway between a stack of hay and a pail of water, and unable to choose between them, dies. In the SSM framework, collapsing decision thresholds (along with noise in the decision process) allow the organism to avert this deadlock. For example, Seeley et al. describe how bee colonies are able to avoid deadlock when deciding between two equally attractive new hive sites[15]. Others have used SSMs to argue that rats, monkeys, and humans utilize collapsing thresholds, urgency signals or non-linear dynamics to avoid indecision[20,60,62–64,69–73]. On the other hand, Hawkins et al. have argued that the evidence for such behavior is not quite so clear, and that it may be present only in extensively trained animals[66].

In any case, even when such mechanisms are present it remains unclear whether they simply serve to break deadlock or whether they produce optimal time allocation. There has indeed been some suggestion that time allocation is not optimal. For example, in the domain of bee color discrimination, researchers have found that in some laboratory settings, bees will overemphasize accuracy and improve their flower color discrimination by an order of magnitude compared to what is typically observed in the field. This enhanced accuracy comes at a substantial time cost; one that is likely higher than the cost of visiting poorly rewarded flowers[52]. Indeed, in an analysis of bee heterogeneity in the flower color discrimination task, Burns found that the fast, inaccurate bees performed better (in terms of nectar collection rate) than the slow, accurate bees[74]. In another example involving a mouse odor discrimination task, the researchers found that their mice exhibited evidence-sampling times that were independent of the difficulty of the decisions, when instead they might have done better skipping through the difficult trials[75]. However, this evidence is limited both in quantity and in the conclusions that one can draw regarding suboptimal behavior.

Our results provide a new source of evidence supporting the view that whatever deadlock-breaking mechanisms exist, they are not maximizing reward rate. While we cannot say whether decision thresholds are collapsing or not, we can say that they are clearly not collapsing optimally. Thus our findings support the view that organisms may be less capable of dealing with these tradeoffs than we might have expected. Future research could utilize a similar choice-deadline procedure to test for sub-optimal time allocation in other species or in highly trained decision makers.

It is worth noting that we purposefully implemented our intervention soft-handedly. We did this to enable subjects to retain agency and avoid being cut off by the computer. While this procedure clearly improved subjects' outcomes, it is unclear whether the intervention worked primarily by serving as a cue to terminate the decision process, or by altering subjects' thresholds. The comparison between pre- and post- non-intervention blocks suggests that in fact the intervention may have had different effects in the two studies. In Study 1 the intervention led to an improvement in non-intervention blocks, suggesting a change in subjects' thresholds, while in Study 2 this effect was not significant. The lingering effect of the intervention even in its aftermath, somewhat weakens the normative case for sustaining the intervention.

Also, while the data show that the intervention enhanced the material benefit of the subjects, it is beyond the scope of the current research to evaluate its subjective benefits, all things considered. For example, it is possible that some organisms assign an intrinsic value to being correct [76]. Indeed, research in signal detection theory has demonstrated that people tend to over-emphasize accuracy[3,4]. If this intrinsic value was higher for hard problems, as research on achievement motivation indeed suggests [e.g. 77], this could explain why subjects might allocate more time to them. However, note that this cannot explain why subjects' behavior improved post-intervention (nor can it explain the lack of a difference between payment schemes documented in Study 3, which is reported in the supplementary materials). Additionally, a common explanation for the over-emphasis on accuracy is that throughout their lives people are reinforced for making correct decisions. However, in our Study 1, there is technically no "accuracy", only consistency with the prior WTP for the different items. In similar "real world" settings, organisms are not typically given feedback about the accuracy of such preference-based decisions, since preferences are subjective.

One might wonder if it matters whether the opportunity costs are certain or uncertain. In Study 1 subjects were compensated for one randomly selected trial and so it is possible that their motivation to optimize performance was reduced. However in Study 2, subjects were compensated for all of their decisions and yet sub-optimal behavior remained. So while probabilistic outcomes may reduce motivations to optimize, they do not seem to be necessary to observe sub-optimal behavior.

Taken together, our research here demonstrates a new, simple way to test for suboptimal behavior. Rather than taking the traditional modeling approach to derive what optimal behavior might look like, we instead used experimental manipulations and behavioral interventions to show that subjects' unrestricted behavior is not payoff maximizing. Of course, it was the modeling literature that first suggested to us that such inefficiency might exist. We thus close with the hope that this work highlights the important complementarities between theory, modeling, and experiments.

Data Accessibility

All data are available on Dryad (http://datadryad.org).

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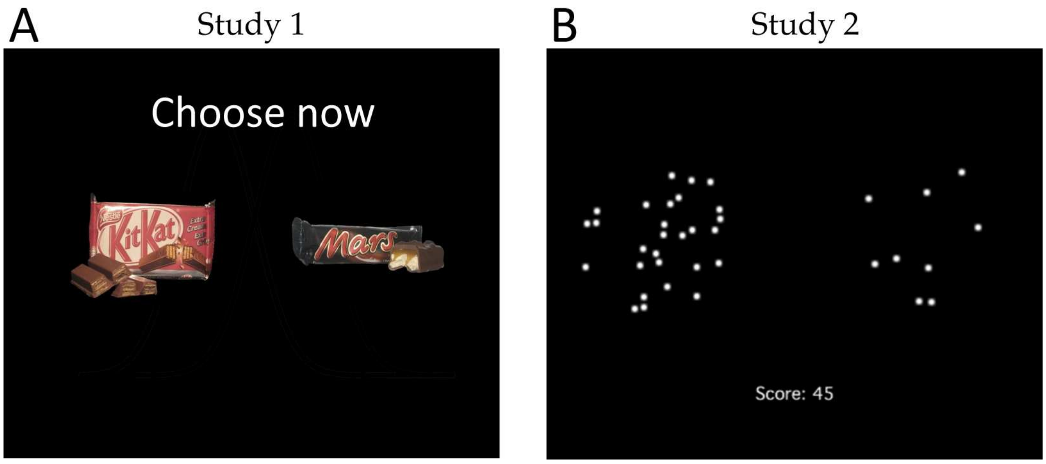
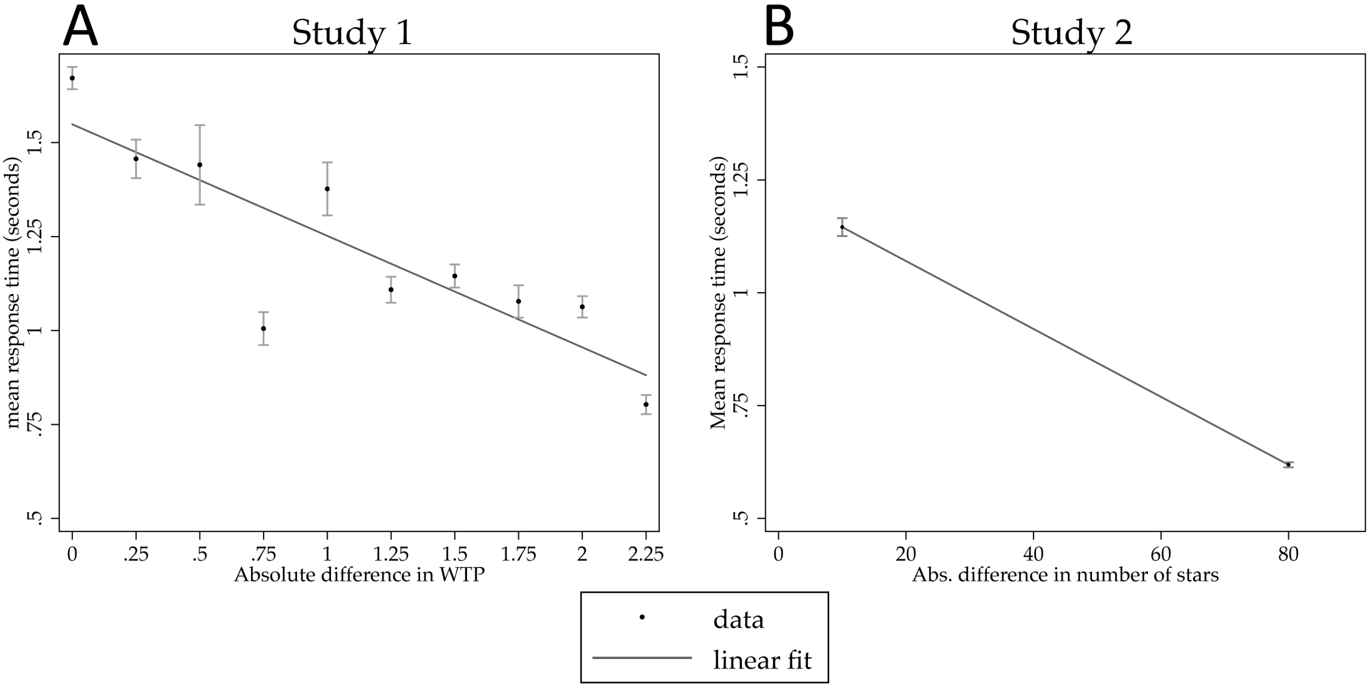
Figure Legends

Fig. 1: Task design (A) Example screen from the value-based food-choice task from Study 1. Subjects simply chose the item that they would prefer to consume at the end of the study. To improve readability, we increased the font and dot size for both panels of this figure. (B) Example screens from the perceptual "twinkling stars'' task from Study 2 (also used in Study 3, which is reported in the supplementary materials). The dots randomly appeared and disappeared, so that at any given point in time only ~80% of them were visible. Subjects had to decide which of the two fields had more dots.

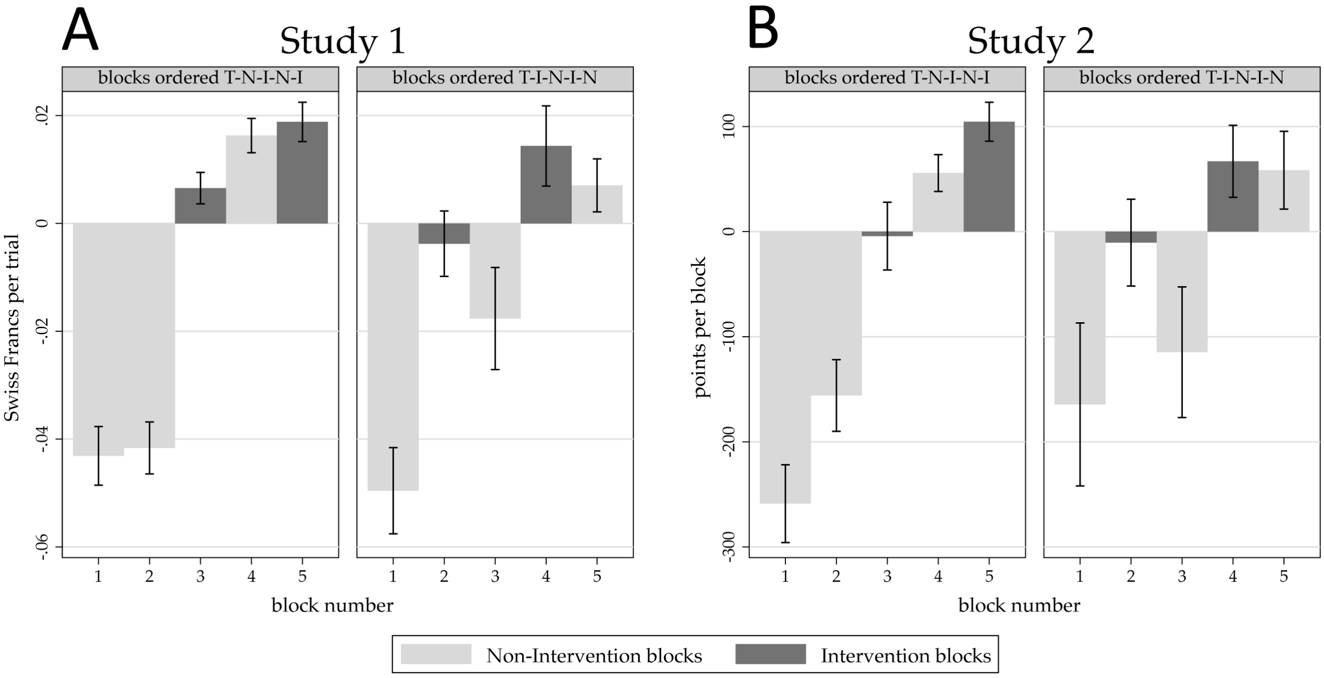
Fig. 2: RT vs. Difficulty. Mean RTs, (black dots with standard error bars) as a function of (A) the difference in willingness to pay (WTP) between the two food items in Study 1 (n=49 subjects), and (B) the difference in the number of stars in Study 2 (n=40 subjects). Choice problems with a low absolute difference in the number of stars or WTP are more difficult and yield lower relative rewards from a correct decision. Although difficult choices benefit less from a correct decision, subjects spend more time on them, which reduces their earnings. Linear fits represent OLS regressions of mean RT on absolute difference in WTP (Study 1), and on the abs. difference in number of stars (Study 2).

Fig. 3: Choice performance by block. (A) Choice surplus earned by n=49 subjects in each of the five blocks. (B) Points earned by n=40 subjects in each of the five blocks. All subjects began with a nonintervention block (first block *T*). In the second block, subjects either experienced another non-intervention (*N*) block (left half-panels) or an intervention (*I*) block (right half-panels). After that, the blocks alternated between *I* and *N.* To better reflect the within-subjects nature of the design, the data were individually de-meaned by subtracting, for each subject, the mean of blocks 2-5. Thus a positive bar means that in this block, on average, participants did better than the average from blocks 2-5, and vice versa for negative bars. Performance in I is always higher than in the previous N trials. The higher performance in I trials also holds when we control for experience.


A
Study 1
Choose now
KitKat
Mars
B
Study 2
Score: 45



A
Study 1
mean response time (seconds)
.5
.75
1
1.25
1.5
0
.25
.5
.75
1
1.25
1.5
1.75
2
2.25
Absolute difference in WTP
B
Study 2
Mean response time (seconds)
.5
.75
1
1.25
1.5
0
20
40
60
80
Abs. difference in number of stars
data
linear fit



A
Study 1
B
Study 2
blocks ordered T-N-I-N-I
blocks ordered T-I-N-I-N
Swiss Francs per trial
points per block
block number
Non-Intervention blocks
Intervention blocks

Irrational Time Allocation in Decision Making

1 Supplementary results for studies 1 and 2

	log(RT)
Abs. difference in WTP between choice options	-0.218***
	(0.000)
Observations	2735
Number of clusters (= number of subjects)	49
Regression constant included	Yes

Table S1: Study 1: Regressions of response times on WTP difference. Mixed-effects regression, estimated by maximum likelihood. Model: $\mathbf{log}(\mathbf{rt_{ij}}) = \boldsymbol{\beta_0} + \boldsymbol{\beta_1}(\Delta\mathbf{v})_{\mathbf{ij}} + \boldsymbol{\eta_i} + \boldsymbol{\varepsilon_{ij}}$, where $\mathbf{j}$ is an index for the trial, $\boldsymbol{\eta_i}$ is an individual-specific noise term for individual $\mathbf{i}$, and $\boldsymbol{\varepsilon_{ij}}$ is a general noise term. Standard errors clustered at the subject level. One observation is one trial of one participant (only human choices). P-values in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Studies 1 and 2: Mixed-effects regressions of material benefit on intervention.

	Study 1: CHF per trial			Study 2: points per block		
	(1)	(2)	(3)	(4)	(5)	(6)
Block number: integer (2 to 5) capturing when block was shown	0.0133*** (0.000)	0.00736*** (0.002)	0.00736*** (0.002)	61.86*** (0.000)	57.78*** (0.000)	57.78*** (0.000)
Nonintervention block (d), excluding block 1	-0.0169*** (0.000)			-76.28** (0.035)		
Nonintervention block, pre intervention (d), excluding block 1		-0.0430*** (0.000)	-0.0355*** (0.000)		-95.90* (0.081)	-26.32 (0.685)
Nonintervention block, post intervention (d)		-0.00747 (0.185)			-69.58 (0.102)	
intervention block (d)			0.00747 (0.185)			69.58 (0.102)
Observations	196	196	196	160	160	160
Number of clusters (=number of participants)	49	49	49	40	40	40
Regression constant included	Yes	Yes	Yes	Yes	Yes	Yes

Table S2: Mixed-effects model: $\boldsymbol{y} = \boldsymbol{\beta x_{ij}} + \boldsymbol{\nu_i} + \boldsymbol{\varepsilon_{ij}}$, where $\boldsymbol{\beta}$ is a vector of coefficients, $\boldsymbol{x_{ij}}$ is the vector of regressors in trial $\boldsymbol{j}$ of individual $\boldsymbol{i}$, $\boldsymbol{\nu_i}$ is an individual-specific noise term, and $\boldsymbol{\varepsilon_{ij}}$ is a general noise term. Estimated using maximum likelihood. Standard errors clustered at the individual level. First block excluded from analysis. For Study 1 (cols 1-3), the dependent variable $\boldsymbol{y}$ is the blockwise mean surplus, in CHF per trial. Since there were 100 trials per block, conversion to the block level requires multiplying by 100. For study 2 (cols 4-6), the dependent variable $\boldsymbol{y}$ is cumulative surplus per block, in points, since every trial was paid in full (1000 points = 1 USD). Before regressing, the data was first collapsed to obtain blockwise mean surplus for each participant, resulting in four data points per participant (representing blocks 2-5). P-values in parentheses. These p-values relate to two-sided tests of the null hypothesis that the respective coefficient is zero. * p < 0.10, ** p < 0.05, *** p < 0.01. (d) signifies binary (dummy) variables.

2 Study 3: Response (or lack thereof) to changes in the reward scheme

2.1 Overview

Studies 1 and 2 conclusively showed that subjects did not optimize their allocation of time in either perceptual or value-based decision making. Instead they appeared to deliberate for too long on low-stakes decisions.

Study 3 used the same basic task as Study 2, but with an additional condition that featured a different incentive structure. This study thus investigated whether subjects would respond to changes in the incentive structure that modify how lucrative it is, comparatively speaking, to invest time into easy vs. hard trials.

In brief, we found no evidence that people respond optimally to changes in the lucrativeness of time investment into easy vs. hard trials. This (null) result, while not conclusive on its own, is consistent with the suboptimality of behavior that we observed in studies 1 and 2.

2.2 Results

Subjects were informed that there were two payment conditions in the experiment. In the fixed pay (F) condition, the amount to be gained or lost was 25 points on every trial, regardless of the number of stars. In contrast, in the difference-based pay (D) condition, the stakes corresponded to the difference in the number of stars between the two sides of the screen, like in Study 2. Since subjects did not know the number of stars on the screen, they had to infer the stake size. We designed this D condition to mirror the link between difficulty and reward in value-based choice in the same way as in Studies 1 and 2.

Thus, in both conditions, easy trials, which could be answered more rapidly, offered higher earnings per unit of time than the hard trials. However, in the D condition, the difference between easy and hard trials was magnified, because in addition, stake size differed by difficulty: easy trials now offered a larger reward for a correct response than hard trials. Thus we predicted that our subjects would adapt by investing relatively less time into the hard trials in the D condition than in the F condition.

After an initial unpaid trial block T, subjects progressed through four more blocks, either in sequence D-D-F-F or F-F-D-D. If we restrict our attention to only the first two blocks (akin to a between-subjects design), we find that the ratio $(median\ rt\ in\ hard\ trials)/(median\ rt\ in\ easy\ trials)$ is not significantly different in the D vs. F treatments (p=0.4657, two-sample rank-sum Mann-Whitney test).

Next, we investigated whether this null finding persisted in our within-subjects data, by analyzing all four paid blocks. Error! Reference source not found. shows, for each subject individually, the change in the ratio $(median\ rt\ in\ hard\ trials)/(median\ rt\ in\ easy\ trials)$, across conditions. Approximately half of the subjects (22/42) shifted in the predicted direction, while the rest (20/42) shifted in the opposite direction. Visually, the magnitudes of the changes were approximately symmetric around zero.

To investigate this statistically, we first ran a Kolmogorov-Smirnov test for equality of distribution functions on the distribution of median response times on hard trials, in the *D* vs. *F* condition, which was not significant (exact p-value 0.346). In addition, we ran a mixed-effects regression, in which we regressed log(RT) on binary variables for trial difficulty and payment scheme to capture main effects, as well as an interaction term between the two (see Table S3). Importantly, the coefficient of this interaction term was not significantly different from zero (z=0.79, p=0.430), so we have no evidence that subjects responded to the change in payment scheme. Only the coefficient for trial difficulty was significantly different from zero (z=-8.85, p<0.001), indicating that subjects completed easy trials more rapidly than hard trials. Analogous tests using mean RTs yield even less significant results, but they require careful treatment of outlier trials and so are presented in a separate section below.

We also investigated subjects' accuracy across trial difficulty and condition. Using a mixed-effects logit model, we regressed accuracy on binary variables for trial difficulty and payment condition, as well as an interaction between the two. We find that in the *D* condition, subjects accuracy did not differ significantly on easy trials (z=1.29, p=0.198), and was smaller in the hard trials (z=-1.97, p=0.049), compared to the *F* condition. The lack of RT effects is thus even more surprising since subjects should devote even less time to lower accuracy hard trials.

Thus, taken together, we find no evidence that subjects adapted their time allocation behavior to the incentive changes in the task. This "null" result does not, on its own, provide conclusive evidence of sub-optimality, but building on the results from Studies 1 & 2, it is consistent with the hypothesis that time misallocation is a pervasive problem.

Robustness check: Analysis based on means, with an alternative treatment of RT outliers

In Study 3, we observed a few trials in which participants' response times were very large (e.g. one participant had a response time of >90s on one of the trials. Since such rare and dramatic outliers are likely caused by distractions or other interruptions of the task, rather than the nature of the task itself, we used a median-based analysis in the main article (as medians are less sensitive to extreme outliers). An alternative way to deal with outliers could be to exclude all data from trials in which response times are >3sd larger than the overall mean. Since the mean (sd) of response times was 1.286s (1.755s), this would affect all trials with RTs larger than 6.551s, resulting in an exclusion of 469/42198 trials (1.11% of the data). The mean-based analogy to the analysis performed in the main article then yields the following results.

If we restrict our attention to only the first two blocks (akin to a between-subjects design), we find that the ratio $(mean\ rt\ in\ hard\ trials)/(mean\ rt\ in\ easy\ trials)$ is 1.156 (n=21, sd = 0.1071) in the *F* treatment, and 1.147 (n=21, d=0.0974) in the *D* treatment. To account for the possible non-normality of the distribution of this ratio, we performed a nonparametric equivalent of the t-test (a two-sample Mann-Whitney rank-sum test), which failed to reject the null hypothesis that the ratios were equal across the two conditions (z=-0.566, p=0.5714).

Next, we investigated whether this null finding persisted in our within-subject data, by analyzing all four paid blocks. Fig. S2**Error! Reference source not found.** shows, for each subject individually, the change in the ratio $(mean\ rt\ in\ hard\ trials)/(mean\ rt\ in\ easy\ trials)$, across conditions. Precisely half of the subjects (21/42) shifted in the predicted direction, while the other half shifted in the opposite direction. Visually, the magnitudes of the changes were also symmetric around zero.

To investigate this statistically, we ran a mixed-effects regression, in which we regressed log(RT) on binary variables for trial difficulty and payment scheme to capture main effects, as well as an interaction term between the two (Table S4). Importantly, the coefficient of this interaction term was not significantly different from zero (p=0.744), so we have no evidence that subjects responded to the change in payment scheme. Only the coefficient for trial difficulty was significantly different from zero (p<0.001), indicating that subjects completed easy trials more rapidly than hard trials.

This alternative analysis thus closely mirrors the results obtained in the median-based analysis that is reported above.

2.3 Discussion

In Study 3, we again made use of the perceptual decision-making task from Study 2, and investigated whether subjects would adjust their time allocation to reflect changing reward schemes. We compared two conditions, one where the points at stake were tied to the difficulty of the task, and one where the stakes were independent of the difficulty level. Yet we found no difference in the fraction of time subjects allocated to hard decisions. We thus do not find any evidence that subjects responded to the change in reward schemes.

2.4 Methods

42 subjects were recruited through a Princeton University online subject recruitment system and provided informed consent to participate in this study. Study 3 employed the same task as in Study 2, but with some notable differences.

The most important difference was that two different payment schemes were used. Subjects were informed that in the fixed pay (*F*) condition, the amount to be gained or lost was 25 points on every trial, regardless of how many stars there were. In contrast, in the difference-based pay (*D*) condition, the stakes corresponded to the difference in the number of stars between the two sides of the screen. For example, if there were 55 stars on the left and 45 stars on the right, the stakes were 10 (since 55-45=10). Since subjects did not know the number of dots in advance, on every trial they had to infer the stakes based on the on-screen stimuli. For half of the subjects, the block-type sequence was *TFFDD*, for the others it was *TDDFF*.

Other changes from Study 2 include the fact that the difference in the number of stars between the two sides of the screen was either 10 (hard trials) or 24 (easy trials), with the mean number of stars equal to 100 (e.g., a “hard’’ trial had 95 vs. 105 stars, and an “easy’’ trial had 88 vs. 112 stars). The points earned corresponded to the difference in the number of stars (i.e. 10 or 24 points per trial). This was motivated by the desire to keep overall payments per subject approximately the same in studies 2 and 3. Finally, the ITI in the paid blocks was 1s, plus the time needed to prepare the next trial, resulting in an average empirical ITI of ~1.1s.

As in Study 2, participants were informed that the study consisted of five blocks of decisions, each of which lasted for ten minutes, and that each block was limited by time, not by number of trials.

Analysis

The mixed-effects regression reported in the results for Study 3 employed the following model: $\log(rt_{ij}) = \beta_0 + \beta_1 easy_{ij} + \beta_2 condition_{ij} + \beta_3 easy_{ij} * condition_{ij} + \nu_i + \varepsilon_{ij}$, where $\boldsymbol{j}$ is an index for the trial, $\boldsymbol{\nu_i}$ is an individual-specific noise term of individual $\boldsymbol{i}$, and $\boldsymbol{\varepsilon_{ij}}$ is a general noise term. As in studies 1 and 2, this mixed-effects regression model was estimated using maximum likelihood. Standard errors were clustered at the individual level. The first (trial) block was excluded from all analyses.

2.5 Figures

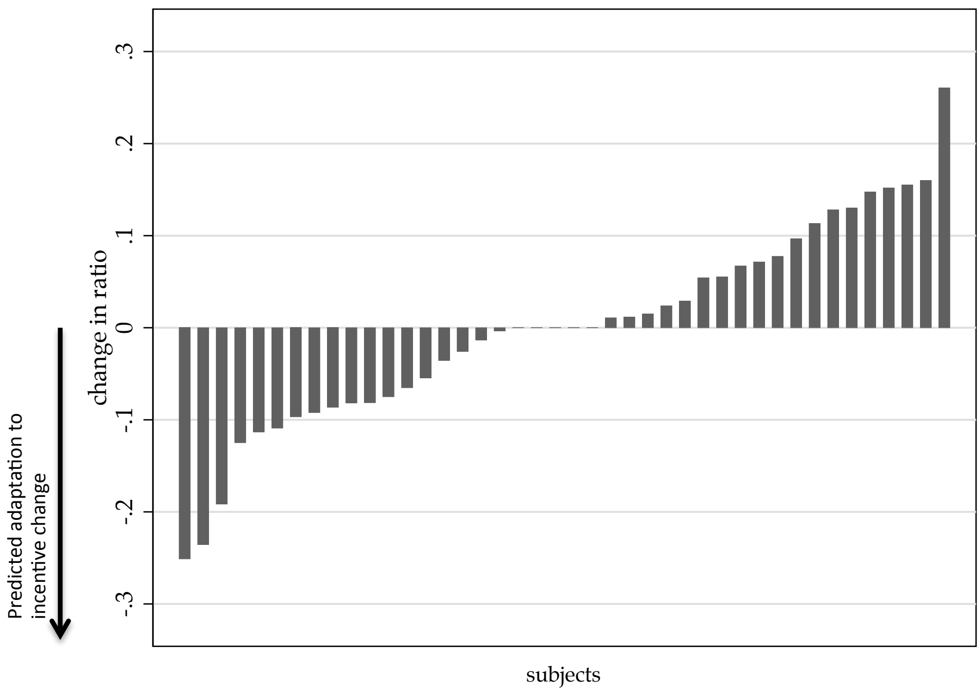


Fig. S1: Study 3 results. Change in the ratio of (median rt in hard)/(median rt in easy) trials for the difference-based (D) pay conditizon minus the fixed (F) pay condition. Each bar represents one subject (n=42 subjects). A positive bar indicates a subject who spent relatively more time on hard trials in the D condition (compared to F), while a negative bar indicates the opposite. If subjects were responding optimally to the incentives in the D condition, they should have spent less time on the hard decisions and more time on the easy decisions, i.e. the ratio depicted in the figure should be negative.

	Study 3: log(RT)
easy trial (d)	-0.117^{***} (0.000)
Difference-based pay (d)	-0.0291 (0.501)
Interaction: easy X Difference-based pay (d)	0.00848 (0.430)
Observations	42198
Number of clusters (= number of subjects)	42
Regression constant included	Yes

Table S3: Mixed-effects regression capturing how log response times respond to condition and trial difficulty in Study 3. Model: $\log(rt_{ij}) = \beta_0 + \beta_1 easy_{ij} + \beta_2 condition_{ij} + \beta_3 easy_{ij} * condition_{ij} + \nu_i + \varepsilon_{ij}$, where j is an index for the trial, ν_i is an individual-specific noise term of individual i, and ε_{ij} is a general noise term. Estimated using maximum likelihood. Standard errors clustered at the individual level. First block excluded from analysis. p-values in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. (d) signifies binary (dummy) variables.

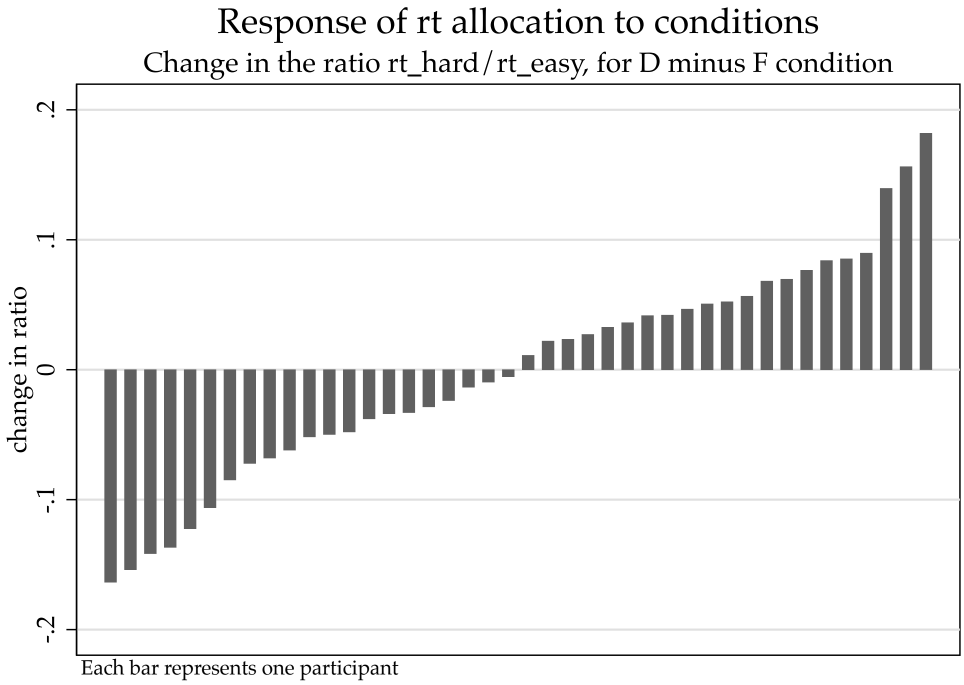


Fig. S2: Study 3 robustness check. Change in the ratio of mean hard / mean easy RTs for the difference-based (*D*) pay condition minus the fixed (*F*) pay condition. Each bar represents one subject. A positive bar indicates a subject who spent relatively more time on hard trials in the *D* condition (compared to *F*), while a negative bar indicates the opposite. Note that trials with RTs > 3sd larger than the overall mean RT were removed as outliers.

	Study 3: log(rt)
easy trial (d)	-0.103*** (0.000)
Difference-based pay (d)	-0.0231 (0.584)
Interaction: easy X Difference-based pay (d)	0.00338 (0.744)
Observations	41729
Number of clusters (=number of participants)	42
Regression constant included	Yes

Table S4 Study 3 robustness check. Mixed-effects regression capturing how log response times respond to condition and trial difficulty, with outliers excluded. Mixed-effects model: $\log(rt_{ij}) = \beta_0 + \beta_1 easy_{ij} + \beta_2 condition_{ij} + \beta_3 easy_{ij} * condition_{ij} + \nu_i + \varepsilon_{ij}$, where j is in index for the trial, ν_i is an individual-specific noise term of individual i, and ε_{ij} is a general noise term. Estimated using maximum likelihood. Standard errors clustered at the individual level. First block excluded from analysis. p-values in parentheses. Excludes trials where $rt > mean(rt) + 3sd$ as outliers. * p < 0.10, ** p < 0.05, *** p < 0.01. (d) signifies binary (dummy) variables.

3 Supplementary Detailed Documentation of Methods

3.1 Study 1

3.1.1 Overview

49 participants first indicated their willingness to pay for 100 different simple snack items, and then proceeded through five blocks of binary decisions between pairs of these items. Each block contained 100 trials, half of which were constructed as “easy” choices (large valuation difference), and half “hard” (small valuation difference). It was virtually impossible to reach all trials in any given block, since each block lasted for only 150 seconds, with 1.5 second intervals between trials, and any choice not made by the decision-maker was instead made by the computer at random. This means that time spent on any particular trial entailed a cost of reducing the number of future trials the individual could reach. Furthermore, after block 1, we implemented a simple intervention in which we imposed a per-trial time limit on half of the four subsequent blocks, based on the prediction that this ought again to cut off predominantly such trials on which a decision-maker dwelled too long on an essentially meaningless choice.

3.1.2 Sample

We recruited N = 49 (23 male, 26 female)[1] participants from the University of Zurich’s participant pool [1]. In order to increase the chance that choices between snack items carried meaning for our participants, our invitations asked participants not to participate if they disliked snack items, and asked them not to eat for four hours before the experimental session. To enhance compliance, all sessions were conducted at 5 pm, i.e. four hours after typical Swiss lunch hours end, but before dinner time. Participants were paid a flat fee of 30 CHF for their participation, plus possible additional cash of up to CHF 2.50 from the first part of the study. Each participant gave written informed consent. The study was conducted from December 2012 to January 2013 in the computer laboratory of the Social and Neural Systems laboratory (SNS-Lab) of the University of Zurich. Ethics approval for Study 1 was obtained from the IRB at the faculty of economics, business administration and computer science of the University of Zurich.

3.1.3 Part 1: Obtaining WTP

In the first part of the study, we recorded participants’ willingness to pay (WTP) for a large number of snack items, using a Becker-DeGroot-Marshak (BDM) mechanism ([2]). This method has the advantage that it is in the participants’ best interest to neither under- nor overstate their WTP. The WTP were later used to construct easy and hard choice pairs, and to establish the potential surplus associated with each choice.

After completing informed consent forms, participants received detailed written instructions (refer to the appendix for a copy), which informed them that at the end of the study, there was going to be a 30-minute waiting phase, during which they were required to stay in the lab, or else they would forego their payment. During this time, they were going to be served any snack item that they may have won during the study. They could either consume it during the 30 minute waiting phase or take it home. The full 30 minute waiting phase applied whether or

[1] In addition to the 49 participants reported here, there was one further male participant (for a total of 50 participants), but unfortunately this participant’s computer crashed during the study, resulting in data loss for this participant.

not one had obtained a snack item. This mandatory 30-minute wait served to further motivate participants to acquire a snack item, as well as give them occasion to consume said item on site.

The measurement of WTP was operationalized as follows. Participants obtained a budget of 2.50 CHF per trial, of which they could bid any amount that they liked for a snack item. For each of 100 snack items, photographs of were displayed one by one on the computer screen.[2] On each screen, participants had to indicate how much they were maximally willing to pay for that item, on a graphical scale from 0 CHF to 2.50 CHF, in intervals of 0.25 CHF, by moving a cursor to the appropriate place using the keyboard. The initial location of the cursor was randomized in each trial, in order to avoid systematic anchoring effects. For a sample screenshot, refer to Figure 1 of the main article. Participants were informed in the instructions that at the end of the study, one trial was going to be drawn and implemented. If this random trial was from part 1 of the study, a random price was determined for the item shown in that trial. If the random price exceeded the stated maximal WTP, then they did not purchase the item, and kept the full 2.50 CHF. If the random price was at or below the stated maximal WTP, they purchased the item at the random price, and retained whatever was left of the 2.50 CHF budget after the purchase. The BDM mechanism is a standard tool for eliciting WTP in economics, because it has the advantage that truth telling is a dominant strategy – i.e. it is always in the participants' best interest to state their true willingness to pay. In particular, it is neither advantageous for a participant to under-, nor to over-state their maximal WTP. The written instructions explained the BDM mechanism in detail, emphasizing that neither under- nor overbidding was in the participant's best interest (see below for a copy of the original instructions).

Participants knew that only one of the trials from either this part of the study or the next was going to be implemented. This served two purposes. First, it meant that at most one snack item could be obtained, and thus ensured that potential interdependencies between different snack items played no role. Second, it served to make participants consider each trial seriously, as each had the potential to be the only one that counted.

Comprehension of the instructions was verified using a series of multiple choice questions that participants filled in after reading the instructions, which had to be correctly answered before beginning the computerized part of the study (see below).

In part 1 of the study, participants moved at their own pace and could take as much time as they liked for each of these evaluations.

3.1.4 Part 2: Five blocks of binary choices

Participants received the instructions for the binary choice phase of the study only after having completed part 1 of the study. The instructions informed them that there were going to be five blocks, each containing 100 decisions between pairs of snack items. They were also informed that the overall time for each block was limited, so it was possible that they were not going to be able to make all 100 choices in each block.

[2] The computerized part of Study 1 was programmed using Psychtoolbox / MATLAB. The source code is available upon request from the authors.

The overall time limit for each block of 100 choices was 150 seconds. In addition, a fixation cross was displayed for 1.25 seconds between any two trials, further reducing the amount of time available for making the choices. Together, this made it virtually impossible for participants to make all 100 decisions in a block.

Critically, participants were informed that any choices they did not themselves make were going to instead be made randomly by the computer, so that in the end all 100 decisions in each block were ultimately going to be made, no matter how many trials participants actually reached. This scarcity of time meant that participants faced a problem of how much decision time to allocate to any particular trial. Since the computer was going to make choices randomly and not according to participants' preferences, there was a high expected gain in choice surplus[3] from making choices oneself in trials where participants have strong preferences between the two items (``easy trials''). In contrast, for trials in which participants are nearly indifferent between the items (``hard trials''), it was going to make little difference which of the two they choose. If they realized that a trial is of low importance, the surplus-maximizing act would be therefore be to quickly randomize and move on, in order to reach more of the important trials. However, the DDM predicts that participants will spend more time on these comparatively irrelevant trials, because they do not have instantaneous access to their latent valuations of the items, and it hence takes them a while to realize that they are close to indifference.

To investigate the effect of hard vs. easy trials, the binary choice pairs were constructed in the following way. Based on the BDM valuations, an algorithm constructed half of the pairs such that the BDM valuations were as far apart as possible (``easy'' pairs). The other half were constructed such that the BDM valuations were as close to each other as possible (``hard'' pairs). These choice pairs were grouped into blocks of 100 trials (50 easy and 50 hard). The sequence of pairs within each block was pseudo-randomized to minimize repeat trials and to ensure a roughly equal fraction of easy and hard decisions across blocks. Participants received no information on how the choice pairs were constructed.

Participants were reminded that one of the trials from the study (either from part 1 or from part 2) was going to be implemented at the end of the experiment, so that they should treat each choice situation as if it were the only one that counted.

3.1.5 Experimental conditions

As in Study 2, the first of the five blocks was used to obtain an individual empirical distribution function for the response times in the task. Four more blocks followed. Two of these were nonintervention (N) blocks, which were constructed identically to the first block, but with different choice pairs. The other two blocks were intervention (I) blocks, in which participants were reminded on screen to "choose now'' after a pre-specified time had passed. If they made no choice within half a second upon seeing the reminder, the choice was randomly made for them, and participants moved on to the next trial. The intervention was thus closely analogous to the intervention in Study 2.

[3] By choice surplus, we mean the valuation of the chosen, minus the un-chosen item. This is a measure of how much valuation is gained by the choice, relative to the un-chosen alternative.

In the intervention blocks, the mean deadline was determined separately for each individual, based on their response times from the first block. As in Study 2, it was defined such that it would have cut off the slowest 30% of their decisions.[4] The actual trial-by-trial deadlines were slightly jittered on a trial-by-trial basis, resulting in a range of cutoff times, which was equivalent to the slowest 17.5-42.5 percent of their first-block response times. The "choose now'' reminder on each trial was timed to appear 0.5 seconds before that trial's deadline.

As in Study 2, the four intervention- and non-intervention blocks were run alternatingly. The sequence was randomly assigned to participants. Thus, for any given participant, there was a 50% chance that they were going to experience the blocks in sequence T-I-N-I-N, and a 50% chance that they would experience them in sequence T-N-I-N-I.

[4] This heuristic threshold was originally determined based on the analysis of the distribution of response times in easy vs. hard trials in the first block in our pilot sessions, with the goal that the cutoff affect primarily hard trials, but not easy trials.

3.1.6 Written instructions and procedural plan

3.1.6.1 Written instructions for Study 1, part 1 (WTP)

Thank you for participating in today's study.

Please carefully read the material on the following pages to understand
- The rules
- The decisions you will be making today

After you have read the instructions, **there will be a few test questions** to make sure you have understood them well.

If you have any questions after reading these instructions or during the experiment, please raise your hand quietly and someone will come and assist you.

The rules:
- Please remain silent during the entire study, remain seated at your place, and refrain from communicating with anyone.
- Please check now to ensure that your mobile phone is switched off. It must remain switched off for the entire duration of the study.
- After the study, there will be a 30 minute waiting period. Depending on your choices during the study today, you will be able to eat food that you have purchased during the study during this time. However, even if you do not acquire any food, you must remain in the laboratory during this time and will not be paid before the 30 minutes are over.
- If you choose to leave early, you are free to do so, but then you will not receive a payment for today's study.

Failure to comply with these rules may result in exclusion from the experiment without pay and removal from our list of participants for future experiments.

About today's study:

Today you will be making a series of choices about different foods. The study will have several parts. You will receive instructions for each of these parts as we move through the study.

At the end of the experiment,
- We will randomly select one of the parts of the study. One of the choices from that part will count. You will then receive additional money and/or a food item to eat, depending on your choice in that round.
- You will be required to stay in the laboratory for 30 minutes, during which time you can consume any food that you might have received in the payment round.

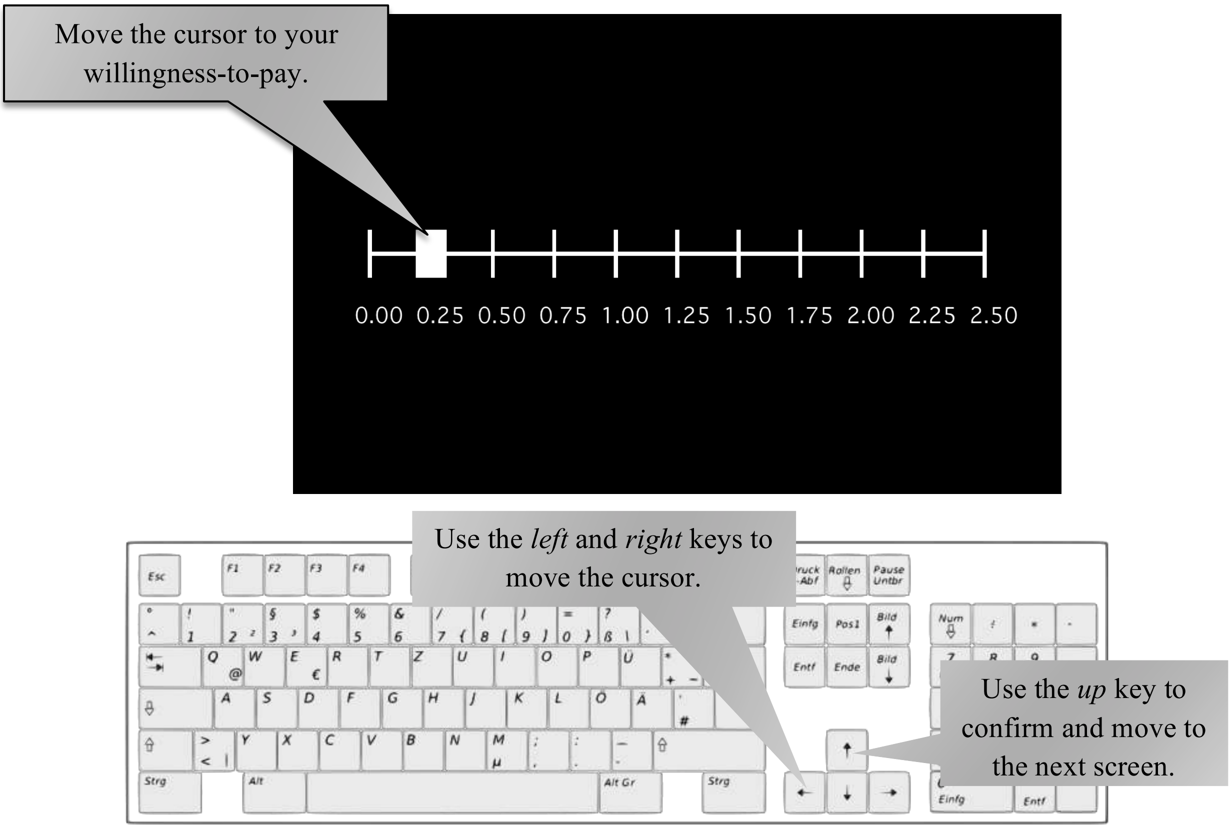
- Even if you did not get any food item, or do not consume the food item, you will be required to remain in the laboratory for the entire 30 minutes. If you choose to leave early, you are free to do so, but then you will not receive a payment for today's study.

Note: It is impossible for us to ensure that every food item is always available. If we do not currently have your selected food item, then we will randomly choose another payment round in which the relevant food item is available.

Part 1: Willingness-to-pay for different food items:

For the first part of the study, you will be shown a series of different foods, one by one. Each food will be displayed on the screen for 2 seconds. You will receive a budget of 2.50 CHF that you can use to buy food. Then you will be asked how much you would be willing to pay to eat that food during the waiting time after the experiment.

To make your choice, use the *left* and *right* arrow keys to select your desired willingness-to-pay and then press the *up* arrow key to enter your decision.



Remember, *only one choice from the whole study* will be chosen at the end for payment so you do not need to worry about spreading your money over the different food items. You should treat each choice as if it is the only that counts.

Also, you will receive the 2.50 CHF budget only if a round from part 1 is selected at the end of the study. If a choice from a later part of the study is randomly selected, then you will not receive the 2.50 CHF.

This part of the study is designed so that **it is always in your best interest to tell us your true willingness-to-pay** for each food item. Below we describe why this is the case.

If one of these rounds is selected at the end for payment, we will pick a random price for the chosen food item, from 0 to 2.50 CHF.

- If the price is less than (or equal to) the willingness-to-pay you gave, then you will buy the food item from us at that price and keep whatever money is left.
- If the price is more than your chosen willingness-to-pay then you will not buy the item and you will keep all 2.50 CHF.

Why should you tell us your true willingness-to-pay?

The thing to notice is that *you cannot affect the price* that you pay for the food. You can only say whether you would be willing to buy the food item at a price that **the computer randomly chooses**. The price that you pay is the random number and NOT your willingness-to-pay. By lowering your willingness-to-pay you will not be able to affect the price that you pay, but might end up losing the opportunity to buy the item at a good price. You should also never raise your willingness-to-pay above your true willingness-to-pay, since then you could end up paying more for the item than you would want to.

For example, suppose that a food item is worth 2 CHF to you. If you reveal your true value, you will get the item if the random price is 0-2 CHF and pay that lower price. In particular, if you had indicated a lower willingness-to-pay, then you would not have paid less. You will not get the food item if the price is more than the 2 CHF you indicated.

Thus, it can never improve your situation if you state a willingness-to-pay that is above or below your true willingness-to-pay.

Later parts of the study

You will receive separate instructions for the other parts of the Study 1t a later point.

Please note that between the different parts of the study, you will notice your screen changing, and may see your computer returning to the Windows desktop briefly. This is normal. In these cases, please wait for the study to continue.

Payment

Your payment will consist of two parts:

- A fixed fee of 30CHF for participating in today's study.
- Any amount of the 2.50 CHF budget that is left, if a round of part 1 is drawn for payment.

Now, please do the comprehension questions on the next page.

3.1.6.2 Comprehension questions for Study 1, part 1 (correct responses underlined)

Please answer the following comprehension questions and raise your hand when you are done. We will then come by your seat and check your answers, and answer any questions you may still have.

When can you leave after the computerized part of the experiment?

1. I can leave right away. In this case I will receive the payment, but no food.
2. I must stay 30 minutes only if I purchase food during the study.
3. <u>I must stay 30 minutes, no matter whether I purchase food or not, if I want the payment.</u>

Which of your decisions will count for payment?

1. Every decision counts.
2. One of the rounds from part 1 is randomly selected for payment.
3. <u>One of the rounds from one of the parts of the study will be randomly selected for payment.</u>

Which of the following statements about part 1 is correct?

1. If I bid nothing on every round in part 1, I will receive 2.50 CHF guaranteed.
2. If a decision from a later part of the study is selected for payment, I will receive 2.50 CHF for part 1.
3. <u>The 2.50 CHF budget applies only to decisions in part 1 of the study. If a different part of the study is selected, I do not receive this budget.</u>

Which of the following statements about part 1 are correct?
Note: *There may be more than one correct answer.*

1. If I indicate *less than* my true willingness-to-pay, I might get lucky and get a better price than if I indicate my real willingness-to-pay.
2. <u>If I indicate *less than* my true willingness-to-pay, this cannot influence how much I will pay if I get the item. This is because the price is determined randomly, no matter what willingness-to-pay I indicate. I only pay this random price if it is less than my willingness-to-pay.</u>
3. <u>If I indicate *less than* my true willingness-to-pay, it could be that the random price is above what I indicated, but below my true willingness-to-pay. This means I will not get the item, even though I could have gotten it for a price below my true willingness-to-pay.</u>

Please raise your hand when you are done with these questions.

3.1.6.3 Written instructions for Study 1, part 2 (binary choices)

Instructions for Part 2: Left-or-right choices

Part 2 of the study is divided into 5 blocks.

Each block contains 100 rounds of choice situations, each on a different screen. On each screen, you will see two food items. Your task is to *choose which of these foods you would like to eat at the end of the experiment*. You can choose either the item on the left or on the right, by hitting the left arrow key (for the left option) or the right arrow key (for the right option). A **white** box will appear around your chosen item and then the next round will begin.

In some blocks the computer will sometimes display the message "Choose now". Once this message has been displayed, you should make your choice immediately. If you do not make your choice within a half-second after the message, the computer will intervene and make a choice for you. You will notice this because one of the items will be selected without you having pressed a key yourself, and then the next round will begin.

Importantly, you do *not* need to wait until you see a message saying "Choose now". You can make your choice anytime and as early as you like, even if you have not seen this message. However, if the message is displayed, you have only 0.5 seconds left.

Remember that at the end of the study, only one of the choices, which can either be from part 1, or from part 2 of the study, will be selected and implemented, and you will receive the chosen item during the 30 minute waiting period before the payments are made. *This means you should carefully consider each choice, and make each choice independently, because it may be the only round that matters in the end.*

Because the time per block is not unlimited, it is possible that you will not have time to reach all 100 decision screens in a block. In this case, the computer will help you by making random choices for the rounds that remain. This ensures that you will receive a food item even if, in the end, a round is chosen that you did not reach.

When you are done with all 5 blocks, the computer will randomly choose a payment round and display your chosen outcome. When this happens, please raise your hand and an experimenter will come and assist you.

Practice Rounds:

Before the first block begins, there will be 5 practice rounds, so that you can become familiar with the way that the program works.

When you have finished reading these instructions, please raise your hand so that we know you are done. We will come to your seat and answer any questions you may have. When everyone is done, we will start part 2 of the study on your computer.

3.2 Study 2

3.2.1 Overview

Study 2 mirrors the payoff structure of Study 1, but with a different task.

3.2.2 Participants

42 subjects were recruited to participate in this study, using the online subject recruitment system of Princeton University's Psychology Department ("SONA''). Participants were members of the Princeton community, mostly Princeton University students. The description provided to participants at sign up was: "You will be asked to make decisions on stimuli presented on screen. Bonus reward based on performance available." All procedures were approved by the Princeton University Institutional Review Board.

3.2.3 Procedure

The procedures implemented in Study 2 were identical to those in Study 3.

3.2.4 Task

Each decision screen showed two black areas, in which dots appeared and disappeared in random locations, much resembling two black fields with twinkling stars. The participants' task was to identify which of the two fields displayed more of the twinkling stars.

The payment in each trial could be either positive (for correct responses) or negative (for incorrect responses), with the magnitude of the payment corresponding to half of the difference (left vs. right) in the number of stars displayed during that trial. Thus, by construction, randomizing produced an expected payoff of zero on any given trial. Due to a typo in the instructions subjects were actually told that they would receive one point for each star, rather than 0.5 points, but note that this should not have had any bearing on subjects' behavior or our analyses, nor did any subjects report noticing the discrepancy. The star difference could be either 10 (hard trials) or 80 stars (easy trials). As a result, the expected gain from correctly responding on an easy trial was higher than the expected gain from getting a hard trial right. Participants earned points cumulatively for each trial they completed, and these points were later converted to USD at a rate of 1000 points = 1 USD.[5]

Since overall time was limited, and each trial was equally likely to be easy or hard, a sophisticated participant should treat the duration of their own decision as informative. The longer it takes to reach a decision, the less likely it is that the current trial is an easy/high stakes trial. Thus if it is not immediately obvious which side has more stars, a maximizer of expected material benefit should be willing to move on before reaching certainty. By accepting a more uncertain, but also more rapid decision, participants would move on to the next trial more quickly, where chances were 50% that this would be an easy and more lucrative high-stakes trial.

[5] Payments were rounded down to full Dollar amounts, and could not be lower than 12 USD, due to university regulations. This gives a payoff function of $Payoff = \max\{\left\lfloor \frac{points}{1000} \right\rfloor, 12\}$. No participant scored less than 12,000 points, and all participants finished the experiment within one hour, such that this restriction was never binding.

Between any two trials, there was an inter-trial time interval (ITI) of 2 seconds, plus the computation time required to prepare the next trial.

3.2.5 Experimental conditions

There were two within-subject experimental conditions: intervention blocks and non-intervention blocks, which will be described in more detail below.

Participants were informed that the study consisted of five blocks of decisions, each of which lasted for ten minutes, and that each block was limited by time, not by number of trials.[6] Participants were introduced to the two experimental conditions in the following way:

"On some runs [blocks], there will be a deadline. If you do not respond by the deadline, the trial will be aborted, and you will earn no points. A short time before the deadline, the stars will disappear - respond quickly when this happens!''

Between any two blocks there was a mandatory break of at least 60 seconds, though participants were free to break for longer if they liked.

In what follows, we will refer to the blocks with deadlines as *intervention* blocks (I), and those without as *non-intervention* blocks (N). The non-intervention blocks were structurally identical to the *difference-based pay* blocks from Study 3, the only difference being that only star differences of 10 or 80 occurred. The first of the five blocks was an unpaid trial block (T), and again served to familiarize participants with the task, and was always a non-intervention block. Its second function was to establish a response time profile for each participant. This distribution of response times was used to construct the deadlines for the intervention blocks. Specifically, the mean deadline was set such that it would have cut off the slowest 30% of responses in the first block. Trial-by-trial deadlines were drawn uniformly from within 50ms of the mean deadline.

As alluded to above, the intervention was designed to harness the informative nature of response times themselves. We hypothesized that by cutting off longer trials, the intervention should affect primarily the hard trials, where it matters least what is chosen, since the subject is close to indifference between them. This should free up decision time, which can then be used to reach more of the easy trials, where it matters more what is chosen. Thus, we reasoned that the intervention should incur only a comparatively small cost on the hard trials, which should be outweighed by the benefit of reaching more of the lucrative easy trials.

To disentangle the effects of the intervention from the effects of mere task experience, the four intervention and non-intervention blocks were run alternatingly, and their sequence was counterbalanced across participants. Thus, a random half of participants experienced the blocks in the sequence T-I-N-I-N, while the other half experienced them in a sequence T-N-I-N-I. Furthermore, since the design also featured non-intervention blocks that followed intervention blocks, it was possible to measure possible spill-over effects of having experienced the intervention in the past, on subsequent non-intervention blocks.

[6] Technically, since there was a ten-minute overall time per block, and there was a 0.5 second interval between trials, the number of trials that could be reached was actually bounded. In practice, however, this bound was never hit by participants.

3.2.6 On-screen instructions

Participants were given the following set of instructions on-screen. After each line of instructions, participants had to press a key on the keyboard to see the next set of instructions. The instructions were the following:

"You will gain and lose points based on your responses. At the end of the experiment, you will receive one dollar for each 1,000 points. If you earn fewer than 12,000 points, however, you will still receive $12."

"The number of points at stake is equal to the difference between the sides. For example, if there are 55 stars on the left and 45 stars on the right, you will gain 10 points for pressing "a" and lose 10 points for pressing "l" (since 55-45=10). If there are 49 and 51 stars, you would gain or lose only 2 points."

"The experiment will consist of 5 runs, each lasting 10 minutes. Each run is limited by time, not by number of trials. The faster you respond, the more trials you will see, and the more money you may earn."

"On some runs, there will be a deadline. If you do not respond by the deadline, the trial will be aborted, and you will earn no points. A short time before the deadline, the stars will disappear -
respond quickly when this happens!"

"Between runs, there will be a break. You are encouraged to take as long a break as you like, but you must take at least, 60 seconds."

"If you have any questions, please ask the experimenter now."

3.3 Study 3

3.3.1 Overview

Study 3 uses the same task as Study 2, with some small alterations, but with an additional condition that featured a different incentive structure. This study was thus designed to investigate whether subjects would respond to changes in the incentive structure that modify how lucrative it is, comparatively speaking, to invest time into easy vs. hard trials.

3.3.2 Participants

42 subjects were recruited through a Princeton University online subject-recruitment system called the Psych Paid Studies blog. The description provided to subjects at sign up was: "You will be asked to make decisions on stimuli presented on screen. Bonus reward based on performance available." This study was run at Green Hall and the Princeton Neuroscience Institute at Princeton University, in July 2013 and January 2014. All procedures were approved by the Princeton University Institutional Review Board.

3.3.3 Procedure

Participants arrived individually. They were provided consent forms and participant information forms to sign, and were then escorted to the experiment room. The instructions were provided verbally, and redundantly on screen for participants to follow along. Participants were informed that they were going to be paid $1 for every 1000 points they earned during the study. For example, a participant with a score of 17232 points would receive 17.00 USD.[7] On average, participants earned 12.95 USD. The experimenter stayed with participants while they read the instructions, in order to answer any questions. Once participants reported they had no further questions, the experimenter left the room and the main part of the experiment began. After participants had completed the computerized study, they returned to the experimenter's office to receive their payment, based on the final score that was displayed on the screen at the end of the experiment.

3.3.4 Task

The computerized part of the study was programmed using Psychtoolbox / MATLAB running on iMac computers. The source code is available upon request from the authors. The experiment was divided into five blocks. In each block, there was a fixed overall amount of time (10 minutes), during which participants were free to spend as much time as they liked on any particular trial.

On each trial, the screen was black, except for two circular areas in which white dots randomly appeared and disappeared, giving a resemblance of "twinkling stars'' (see screenshot in Figure 1 of the main article).

Possible star locations within each field were drawn randomly, under the constraint that no two stars could overlap. During each 50ms timestep, each star was either lit or unlit. An unlit location had a 40% chance of becoming lit in the subsequent timestep, while a lit location has a 10% chance of becoming unlit. This means that the stimulus was inherently noisy, and on average 80% of the stars would be lit at a time.

Participants' task was to indicate whether they thought the left or the right side of the screen contained more stars, using two buttons on the computer keyboard. As soon as they had hit one of these buttons, the trial ended. Between any two trials, there was an inter-trial time interval (ITI) of 1 second, plus the computation time required to prepare the next trial.

3.3.5 Experimental conditions

After the instructions had been read on screen and all questions had been answered, an unpaid *trial block* (*T*) began, in order for participants to familiarize with the task. Four paid blocks followed. As alluded to above, there were two types of blocks – those with *fixed* (*F*) or *difference-based* (*D*) payment schedules. For half of participants, the block-type sequence was *TFFDD*, for the others it was *TDDFF*.

On each trial, participants could either gain or lose a specific number of points, which we will call the "stake'' for that trial. Participants were informed that in the *F* condition, the stake to be gained or lost was 25 points on every trial, regardless of how many stars there were. In contrast, in the *D* condition, the stake corresponded to the difference in the number of stars on the left vs. right side of the screen of the current trial. For example, if there were 55 stars on the left and 45 stars on the right, the participant would gain 10 points for pressing the key corresponding to the left and lose 10 points for pressing the key corresponding to the right (since 55-45=10). Since participants did not know the number of dots in advance, on every

[7] Due to binding university regulations on minimum participant compensation, we had to ensure that minimum earnings of 12.00 USD per hour were guaranteed. This meant that subjects earned 12 USD per hour even if they earned less than 12,000 points.

trial, they had to infer these stakes based on the stimuli on screen. The difference in the number of stars displayed in the left. vs. right half of the screen in Study 3 could be either 10 or 24.

3.3.6 On-screen instructions

Participants were given the following set of instructions on-screen. After each line of instructions, participants had to press a key on the keyboard to see the next set of instructions. The instructions were the following:

"In each trial, press "a" if there are more stars on the left, and "l" if there are more stars on the right. Press any key to see a sample now." (This was followed by an example stimulus that lasted until the subject pressed a key).

"You will gain and lose points based on your responses. At the end of the experiment, you will receive one dollar for each 1,000 points. If you earn fewer than 12,000 points, however, you will still receive $12"

"The experiment will consist of 5 runs, each lasting 10 minutes. The first run is for practice and is not worth any points. In the remaining runs, you will receive points in a way that will be explained."

"Between runs, there will be a break. You are encouraged to take as long a break as you like, but you must take at least 30 seconds."

The following instruction was provided prior to the fixed payment blocks: *"In this part of the experiment, you will receive 25 for each correct response, and lose -25 points for each incorrect response."*

For the difference-based payment blocks, the following instruction was provided: *"In this part of the experiment, the number of points at stake is equal to the difference between the sides. For example, if there are 55 stars on the left and 45 stars on the right, you will gain 10 points for pressing "a" and lose 10 points for pressing "l" (since 55-45=10). If there are 49 and 51 stars, you would gain or lose only 2 points."*

"If you have any questions, please ask the experimenter now."

4 References

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